f.15

AQUILEGIAS.

There are not many flowers that possess higher qualifications than these for garden cultivation. I am greatly surprised that their culture should be comparatively limited in Scotland, where they can always be grown successfully. They are for the most part hardy, vigorous in growth (especially the hybrids from A. cœrulea and A. chrysantha), and highly decorative in general effect. Many of them are contemporaneous in their period of bloom—such, for example, are the "Canadian" and the "Golden" Columbines, which are, in my opinion, the most reliable, hardiest in character, and longest lived of all. They are not less valuable than the fragrant Viola and the many-coloured Spanish Iris for artistic value; but, unlike those somewhat evanescent flowers, they survive for many years even in situations where other more tender plants would soon die.

Aquilegias, like many other alpine and herbaceous plants, have a wide distribution. They are chiefly discoverable among the loftier mountain ranges of Europe, Asia, and America. We need not, therefore, be surprised at their capability of enduring the great severity of frost, when we remember the regions whence they have come. Indeed, it may be said that so far as regards such truly alpine forms as A. glandulosa, which finds itself at home in the Highlands of Scotland, a comparatively cold climate is requisite for their success. It has been asserted by several writers on Aquilegias that many of the finest varieties of the plants require to be treated as biennials, and grown periodically from seed to ensure perpetuation; but I find that this is not necessary, in my part of the country at least, for the continued existence of A. cœrulea hybrida, A. canadensis, or A. chrysantha, which, so far from perishing prematurely, like their unreliable associate, A. glandulosa, after one short season of limited bloom, seem to grow stronger and more massive every year. It is possible that they might be found more transitory if grown further south; yet I cannot but remember that some of the grandest hybrids from cœrulea, chrysantha, and other notable varieties have been cultivated as perennials by the Messrs. Veitch of Chelsea, Mr. Thomas Francis Rivers of Sawbridgeworth, in Hertfordshire, and Mr. Harry Turner of the Royal Nurseries at Slough, all of whom are enthusiastic admirers, like myself, of this Orchid-rivalling flower.

It is somewhat remarkable that the Scottish cultivator, who has given us the most vigorous and the most enduring, also, perhaps, the loveliest and most fragrant of Violas—the miniature, rayless variety, entitled Violetta—should also have originated the most evanescently beautiful of all Aquilegias, viz., A. Stuarti, a highly interesting hybrid (as I am informed by the raiser), between A. glandulosa and A. Witmanniana, which requires to be treated invariably as a biennial, and cultivated very carefully in deep, fertile soil. It was, I understand, named by the late Professor Balfour at Edinburgh, and introduced into cultivation by Messrs. Cocker of Aberdeen.

I am free to confess that, notwithstanding all my efforts, I have not hitherto been very successful with Aquilegia Stuarti, probably because the best situations in my garden having been previously occupied by Roses, Tigridias, Irises, Narcissi, and Eastern Lilies, I was unable to give it a sufficiently-rich soil; and though I have had it occasionally in flower, I have found that its blooms, however imposing in dimensions and artistic in aspect, are comparatively rare. Nevertheless I affirm, that notwithstanding the limitations indicated, it is well worth growing for its attractiveness. It is a distinct advance on Aquilegia glandulosa.

One of the finest and earliest in flowering of the Californian varieties is A. cœrulea hybrida, manifestly a cross between A. cœrulea and chrysantha; for the former, which precedes this fine hybrid by nearly a fortnight, is entirely destitute of any primrose hue, which consequently was the result of cross-fertilisation. This, possibly, may have been the work of insects. Here this variety, not less than its parent, is a true perennial, demanding little attention in summer, no protection in winter, and showing no diminution in vitality at the end of ten years. Aquilegia Skinneri, a native of the northern regions of South America, I have not found so vigorous as A. californica or A. canadensis, of which the former, with its brilliant scarlet hues, is unquestionably one of the showiest of all the American Columbines. It is even finer than A. canadensis, which, I am informed, originally came to this country from Virginia. Its grows abundantly in the North American forests, and generally in extremely rocky situations. It is, I may incidentally observe, a variation from this interesting flower, of considerably stronger constitution than the original, which is generally cultivated in European gardens. I have not found it under any circumstances or climatic conditions arduous of cultivation, having experienced much more difficulty with other varieties, such as A. alpina superba, A. glandulosa, and—as I have indicated—A. Stuarti. Among the most beautiful of recent hybrids are those which have been raised and introduced by the Messrs. Veitch; many of these have a very distinguished appearance, quite unlike that of their predecessors, for which reason they are worthy of general cultivation.

A celebrated writer on Rose-culture has somewhere said, that if he were strictly limited by any unkind fate to one variety of the Rose, he would choose that most vigorous and most prolific of all climbing varieties, Gloire de Dijon. If I were similarly restricted with reference to Aquilegias, I would, without hesitation, and for similar reasons, select the beautiful Aquilegia chrysantha. A plant stronger in constitution, more exuberant in growth, more luxuriant in flowering, and more singularly artistic in floral formation, and in aspect, for garden ornamentation does not exist. *David R. Williamson.*

AMERICAN NOTES.

NEW VARIETIES OF FRUITS.

The report of the Pomologist of the United States Department of Agriculture for 1895 has just been distributed, and though as tardy as any Government document, it is worth notice. As usual, the matter of greatest importance is the report on "Promising New Fruits," under which head are described the various Apples, Pears, Plums, &c., which have been recently introduced and brought to the notice of the Division of Pomology. The list includes 100 varieties of Apples, mostly quite new, 13 varieties of Pears, 3 of Apricots, 11 of Cherries, 25 of Peaches, 17 of Plums, 2 of Grapes, 4 of Oranges, and 1 of Pomelo. This record for a single year indicates that the American fruit list is increasing with all desirable rapidity. Of course, these records represent a very large country, with many very diverse conditions, wherein a considerable number of these varieties achieve only a local importance. But it is easy to see that our fruit list will soon become so tremendous as to frighten the conscientious student of pomology; and a complete revision of Mr. Downing's book has

which means only that American seedlings are proving to be better adapted to local conditions than their parent varieties imported from Europe. And that is only what might have been expected.

Forest Reservations.

A strong fight is being made at Washington against the large forest reservations made by President Cleveland during the last days of his administration. Cattle-men, miners, railroad men, and many other private interests with which the reservations interfere, are bringing every political influence to bear on President McKinley and on Congress to have the order rescinded. It seems probable at the present time that some concessional modifications may be made in the executive proclamation of reservation, but it is hoped and believed that no general abrogation of the order will be attempted. *F. A. Waugh, Burlington, Vermont, April 28.*

METHODS OF PROPAGATION.

(*Continued from p. 285.*)

Conifers from Seed.—If only a few trees or shrubs of any kind are required, and it is desired to raise them from seed, sow the seed in boxes or seed-pans, and place them in a cold pit, where they should be watered as often as necessary. Transplant the seedlings into the quarters as soon as they are large enough, and thus prevent the roots becoming matted together. Conifers are always best when raised from seed; they can then develop naturally to the acute pyramidal form, that makes these trees so useful to the landscape gardener. When planted for ornamental purposes, they should never be crowded, but if grown for timber only, it is necessary to plant them closely, as they draw one another up, as it is called, and may be thinned out when they reach the desired height by simply cutting down, as they do not, like many other trees, "stool," or throw up young growth from their roots. The cones generally do not attain maturity till the second year after forming. The structure of the wood, as viewed in section under the microscope, is unique, the pitted cells, which are always present, determining at once that the tree from which the section has been cut was a Conifer. The most important northern timber trees belong to this order, but a few only are grown largely in this country as forest trees, the great bulk of the Fir timber being imported from Norway and Sweden, from Canada, and other parts of the New World. But this need not be a permanent condition, as thousands of acres of land which are now almost idle might be profitably employed to grow this class of timber. The seeds of all the genus are securely imbedded in the cones, and in some cases are very difficult to free.

The cones should be collected in late autumn, and stored in a dry warm place, in boxes, such as that over the hot-water boiler, where the temperature is regular and dry. Thus treated the scales of the cone will split open, and the seeds will generally be freed, if the cone be taken in the hand and struck sharply at its point on some hard substance. Having fanned away the scales and abortive seeds, the rest may be sown in light peaty soil, in an outside pit, protected in bad weather by reed or straw movable lights, or if there is much quantity the seed may be sown as

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employed, its quality should admit of clear definition, so that the experiment could be simultaneously carried on by different persons, and be hereafter repeated under precisely similar conditions, so far as that important element is concerned. The second desideratum is not immediately felt, as it relates to the possibility of future experiments of the same general character being hereafter made on numerous different plants, in which case it would be well to employ a limited number of different and well-specified soils, or perhaps only two of them, a light and a heavy, with possibly the occasional mixture of some definite dose of a chemical ingredient. A reasonable method of meeting the difficulty would be to obtain the soil annually from localities well known for their horticultural and chemical peculiarities. Therefore, the second enquiry that I make is, What well-defined soils would be suitable for these experiments?

I SHOULD be grateful if any of your correspondents would kindly give advice on the details of an experiment I have in prospect. Suggestions are more especially desired as to the most suitable plants for the purpose.

The experiment is intended to be carried on by a process of "backward selection," or in the opposite direction to that followed by breeders, whether of plants or animals, when they attempt to create a new variety. They select for the parents of each coming generation those individuals of their experimental stock, whose characteristics approach most nearly to the ideal type pictured in their imagination. My aim is the very reverse of this: it is to begin with a variety that has become established, and to breed back to the original form. The primary object is to learn the number of generations that must elapse before the original form is reached, under specified conditions of culture and selection. By this process it is hoped that a practical means of measuring the stability of strains, varieties and races, may ultimately become systematised, that more light will be thrown on the steps through which changes of type take place, and that many matters of high theoretical importance may be cleared up, relating to the distribution of variations and to the varying degrees of continuity or discontinuity in regression, which are too technical to be discussed here.

Whatever may be ultimately done in this direction, it seems clear that the earlier attempts should be conducted under the easiest conditions, and especially by employing the plant that seems best adapted to the purpose. The principal desiderata are: that it should be a hardy annual, in extensive cultivation, consisting of an original race, and of a distinct and well-established variety that has been recognised for a considerable time. Also that the plants, both of the original race and of the variety, should admit of being grown in a healthy state, in small flower-pots. It would further greatly facilitate the experiment if the main difference between the race and the variety lay simply in their sizes, the one being a dwarf form of the other. Anyhow for the first trial, a plant ought to be employed in which the differences are, in some way, strictly measureable. Units of length are serviceable for height of plant and for length and breadth of leaves, &c.; units of number, for number of leaves, spots, serrations, &c.; units of time, for period of sprouting, budding, &c.

The first enquiry that I make is, What plants best fulfil the above requirements?

Next, as to the soil in which to grow them, for the tendency of a variety to relapse into its original form greatly depends on the character of the soil. There are two desiderata to be fulfilled. The first is, that whatever soil be

There are many other details of procedure that require to be determined, referring to mode of planting, exposure, watering, avoidance of cross-fertilisation, &c., which could no doubt be clearly systematised on a carefully-considered plan, so as to ensure uniformity of treatment by different experimenters, but I will not at present ask particularly about these.

Assuming that we have fixed on a plant of the original stock R, and on its variety V, severally planted in suitable and specified soils, and that the experimental series X, planted in the same soil as R, is intended to change V back into R, the proposed experiment would be something of the following form:—There would be a few, say a dozen, specimens of both R and V, and fully 100 of X, each planted in a separate flower-pot, requiring the use of some 124 pots altogether. R and V would be annually raised from seeds procured from the same seedsman, to serve as references, for they and the experimental set would be equally affected by the varying peculiarities of the climate, &c., in different years, as well as by the permanent environments of the locality.

For simplicity of explanation, let us suppose the noticeable difference between R and V to consist in their height at the time when they begin to bud, V being a dwarf variety. Also that the change backwards occurs gradually, and not by sudden jumps on the part of individual plants. Some days before the expected period of budding, a provisional attempt would be made to so arrange the pots that the plants shall stand in orderly sequence, beginning with the shortest, and ending with the tallest. The pots of R and of V would be arranged on the same principle. When the buds begin to show, the orderly arrangement of the three sets would be carefully and finally revised, and the class-place of each plant in its respective series would be chalked on its pot, No. 1 signifying the lowest place. Consequently the two middlemost of the R series would be 5 and 6, the two middlemost of the V series would also be 5 and 6, while those of the X series would be 50 and 51. A single class-place makes little difference except towards the extreme ends. The next step is to see by direct comparison whether R 5 or R 6 coincides in height with any one of the V series, placing them on either side of it. In the first year, probably, the middlemost of the R would be taller than the tallest of the V set. In that case, set aside the, say, five tallest of V, viz., Nos. 96 to 100 for seeding, and pinch off every bud from every other plant of all the sets, so that no risk of cross-fertilisation may subsequently

the shelves to give the scale of the photograph. By these means every desirable measurement of the plants admits of being leisurely made by the statistician, who will treat his measurements according to modern methods, and deduce the required information from them.

Proceeding year after year in this way, the mean height of X will increase, but it would be inadvisable to wait until the middlemost plant of X closely coincided with the middlemost of R. The increase in height of X may be very rapid during the earlier years, but will become gradually slower, and at length so slow that close coincidence will not occur for a long time; and, again, when it does so, the precise epoch could not be determined with confidence. It would be much better to complete the experiment at an earlier stage. That which I would propose is the first year on which the X-plant, which occupies the 75th class-place, in a series of 100, fairly coincides in height with the middlemost of R. Technically, this would be termed the "upper quartile" X-plant, because it stands one-quarter of the way down from the top-end of the class. The height of the upper quartile plant (as of that occupying any other specified class-place), is independent of the number of plants in the series, so long as they are sufficiently numerous for statistical purposes. The upper quartile is very easily ascertained, whatever the number of the plants in the series may be; it is an easily-remembered class-place, and it is one that plays an important part in the higher methods of statistics. Had V been a large variety, and not a dwarf variety of R, the lower quartile, or the twenty-fifth plant in a series of 100, would have been employed.

It is hardly necessary here to speak at length on such changes of the process as would be needed in the very probable event of some few of the X-plants making a sudden change to R, because the reader can easily foresee them.

The process just described, except the photographic part of it, is not restricted to single and measurable characteristics, but is generally applicable, so long as the individuals admit of being classed in orderly sequence, whether by measurements, by intercomparison, or by marks awarded according to the judgment of an examiner. Thus, when plants, or animals, are submitted to prize competitions, the judges have to take simultaneous note of numerous "points," and to give their marks and classify accordingly, and they do so with fair precision, as shown by the accordance between the judgments of different experts. Therefore, although the measurement of a single character and the

Cases undoubtedly exist of populations descended from a single pair, but these are exceptions to a general rule, for breeders of all kinds of domestic stock & plants concur in testifying to the following common experience. Namely that if a pair be selected to breed from, however healthy they are and however fertile in healthy offspring they may prove, yet those offspring (if they are interbred) will produce a degenerating stock. If however a new strain of blood be introduced it will have an immediate & most marked effect in rehabilitating the physique of the succeeding generation.

In order to grasp the significance of these facts more surely, without entangling ourselves among questionable details, it will be convenient to make use of the word biophor introduced by Weissmann & that of stirp which I myself have employed. By biophor is meant

By stirp is here meant the sum of the hereditarily effective portion of the contents of the parental sperm & germ cells at the moment of their conjugation. It is therefore the source from which all the material is derived that will or could be used in the formation of the child, both in respect to its person (or soma) and to its cognate germinal elements, from which the next generation will be produced [I prefer not to tie myself to the word germ-plasm on account of its many connotations)

The significance of the experience of the breeder is then explained by saying that the somatic elements in each child of a healthy & unrelated couple are usually healthy & complete, but that the germinal elements of those children are not so, for if they are interbred they produce a degenerating stock. Consequently they are ineffective or deficient in some common particular. The germinal elements contain therefore a less complete and efficient assortment of the biophors than its cognate soma. There seems to be no way of escape from this conclusion and that no theory of heredity can be considered complete that does not fairly meet and fully explain this curious inference.

It seems to me that a competition, so to speak, must exist between the soma and the germinal elements for the possession of an adequate supply of each of the kind of biophor and that the soma usually gets the best of it in the scramble & the germinal elements being supplied from its leavings. It is reasonable to believe that the organisation of the soma is the more exacting & rigorous of the two competitors, that is to say, it is shaped by stronger attractions & repulsions than those concerned in its [illegible]. It may also well happen in some cases the needs of the soma may absorb an over large proportion of the biophors of some particular kind leaving an insufficient supply of that kind for the germinal elements, or even none at all of them.

There is additional evidence that the soma & the germinal elements are rivals, as in those cases where a disease or other peculiarity skips a generation (Darwin ...)

It will clear the ideas as to the effect of the hypothesis that I offer, if we consider a simple and imaginary case which shall however bear a sufficiently close resemblance to those that may actually occur in nature to serve the purpose. Suppose then a [illegible] cell containing approximately thousands or many hundreds of thousands of different kinds of biophors and that the chance is very small that any one specified kind is imperfectly represented in it, but a great chance that some one kind should be so, & consequently [illegible] [illegible] the germinal elements exceed the soma of the next [illegible]. Under these circumstances parthenogenesis would be inadequate to maintain the race however much the other conditions might be modified for the purpose (It would for example be of no help if the Ids in an Idant were double in number).

It appears then that the strange complexity of bi-sexual generation is due to the fact that the germinal elements suffer for the good of the soma.

(In bi-sexual generation the complete stock arising from it would be very improbable that the same kind of biophor should fail in the contributions of both parents.)

How then, it will be asked, can parthenogenesis be possible in any case, as a regular means of production? A two fold answer suggests itself: First the organisation of the creatures among whom parthenogenesis occurs, is so greatly inferior in its complexity to that of the animals above referred to, that the organisation of the germinal elements may be much stronger, nearly as strong as that of the soma. It would not then suffer from robbery as much as even the higher animals. Secondly we are not well acquainted with the lines of descent of parthenogenetic creatures as is desirable. Their progeny is enormously large and it may well be that their populations are due to the descendants of a comparatively few exceptional specimens (as in the case mentioned at the outset of these remarks), the stock of the remainder dying out.

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F. Galton

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On the greatest number of ancestral relationships of like degree, to a member of a population that has sprung from a single couple.

The problem in its simplest form requires the following suppositions.

(1) the popⁿ increases in successive generations, geometrically, with a constant coeffᵗ r

(2) When r is fractional, it is clear that the first generation, at least, is subjected to an absurd hypothesis, for a single couple cannot have a fractional number of children. As ~~soon~~ as the popⁿ is of moderate size all that difficulty disappears, while the difficulty may be neglected in the early generations without sensible error in the result.

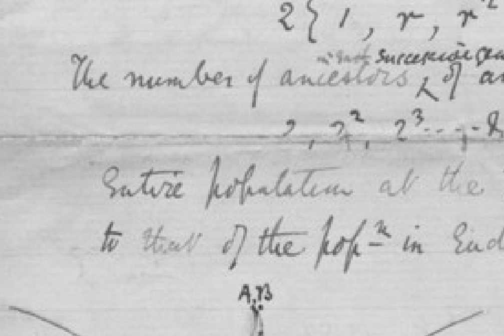
3) Marriages between cousins are supposed to be prohibited, after the first start.

The number of individuals in each successive generation downwards will be

$$2\{1,\ r,\ r^2 \ldots\ldots ,\ r^n\}$$

The number of ancestors in each successive generation of an individual Z, in the n^{th} generation, reckoning upwards, will be

$2,\ 2^2,\ 2^3 \ldots$ &c up to 2^s which is equal to the number of the entire population at the time; after this the ~~above~~ number of ancestors will correspond to that of the popⁿ in each successive ascending degree up to the 2 original progenitors, A, B.



The greatest number of ancestral relationships of like degree occurs at the level DE where the ascending curves intersect the descending ones. Let this level correspond to that of generation s. The number of ancestral relatˢ there is obtained from the equation

$$2 \times r^s = 2^{n-s} \quad \text{or} \quad r^s = 2^{n-s-1}$$ wh: when n & s are large and very unequal becomes $r^s = 2^{n-s}$

$$\text{or} \quad s \log r = \overline{n-s} \cdot \log 2$$

$$\text{whence} \quad \frac{s}{n} = \frac{\log 2}{\log r + \log 2}$$

$\log 1.5 = .18$
$\log 2.0 = .30$

$r = 2 \qquad \frac{s}{n} = \frac{1}{2}$

$= 1.5 \qquad = \frac{15}{24} = \frac{3}{5}$ nearly

$\frac{30}{18+30} = \frac{30}{48} = \frac{15}{24}$

The total number of the ancestry of Z $= 2\{1 + r + r^2 + \cdots + r^s\} + \{2 + 2^2 + 2^3 + \cdots + 2^{n-s}\}$.

$= 2(s+1) + (r^s+1) \ldots + (n-s+1) \ldots$

(80)

On the greatest possible variety & greatest number [?] of ancestry in a specified later [?] generation [?] descended from a single pair

Number of ancestors ?

Possible distribution of Ancestry in a popn [?] descended from a single pair.